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HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Biochemist Waldo E. Cohn, Ph.D.

Conducted January 18, 1995

United States Department of Energy
Office of Human Radiation Experiments
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FOREWORD

IN DECEMBER 1993, U.S. Secretary of Energy Hazel R. O'Leary announced her Openness Initiative. As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects. The Office of Human Radiation Experiments coordinated the Department's search for records about these experiments. An enormous volume of historical records has been located. Many of these records were disorganized; often poorly cataloged, if at all; and scattered across the country in holding areas, archives, and records centers.

The Department has produced a roadmap to the large universe of pertinent information: *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records* (DOE/EH-0445, February 1995). The collected documents are also accessible through the Internet World Wide Web under `http://www.ohre.doe.gov`. The passage of time, the state of existing records, and the fact that some decisionmaking processes were never documented in written form, caused the Department to consider other means to supplement the documentary record.

In September 1994, the Office of Human Radiation Experiments, in collaboration with Lawrence Berkeley Laboratory, began an oral history project to fulfill this goal. The project involved interviewing researchers and others with firsthand knowledge of either the human radiation experimentation that occurred during the Cold War or the institutional context in which such experimentation took place. The purpose of this project was to enrich the documentary record, provide missing information, and allow the researchers an opportunity to provide their perspective.

Thirty audiotaped interviews were conducted from September 1994 through January 1995. Interviewees were permitted to review the transcripts of their oral histories. Their comments were incorporated into the final version of the transcript if those comments supplemented, clarified, or corrected the contents of the interviews.

The Department of Energy is grateful to the scientists and researchers who agreed to participate in this project, many of whom were pioneers in the development of nuclear medicine. ❑

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DISCLAIMER

The opinions expressed by the interviewee are his own and do not necessarily reflect those of the U.S. Department of Energy. The Department neither endorses nor disagrees with such views. Moreover, the Department of Energy makes no representations as to the accuracy or completeness of the information provided by the interviewee.

ORAL HISTORY OF BIOCHEMIST WALDO E. COHN, Ph.D.

Conducted on January 18, 1995, in Oak Ridge, Tennessee, by Thomas Fisher, Jr. and Michael Yuffee from the Office of Human Radiation Experiments, U. S. Department of Energy.

Waldo E. Cohn was selected for the oral history project because of his early research at the Oak Ridge National Laboratory, where he investigated the radiotoxicity of fission products; his role as the architect of this country's postwar isotope production and distribution policy; and his work developing a technique—ion-exchange chromatography—that has proved invaluable in the study of nucleic acids and other areas. The oral history primarily covers Dr. Cohn's early involvement with the Manhattan Project, including his work as the Biochemistry Group leader at the University of Chicago's Metallurgical Laboratory and his tenure at Oak Ridge National Laboratory, where he was a senior biochemist in the Biology Division.

Short Biography

Dr. Cohn was born in San Francisco, California, on June 28, 1910. He received his B.S. in 1931, his M.S. in Chemistry in 1932, and his Ph.D. in Biochemistry in 1938, all from the University of California, Berkeley. His graduate research involved the use of cyclotron-produced radioisotopes, and was thus among the first investigations in the U.S. to use these materials as tracers in uncovering metabolic and physiological processes. He has been married twice and has two children.

Dr. Cohn began his career as a teaching and research assistant in Biochemistry at Berkeley (1937–39). From 1939 to 1942, Dr. Cohn continued his research at Harvard Medical School. In 1942, Dr. Cohn was appointed Section Chief, Chemistry Division, of the Metallurgical Laboratory at the University of Chicago, where he stayed one year before moving to Oak Ridge. In 1947, Dr. Cohn became Senior Chemist and Group Leader of the Biology Division at Oak Ridge, a position he held until his retirement in 1975. While at Oak Ridge, Dr. Cohn has held the following positions:

- 1955 to 1956—Fulbright Scholar, Cambridge University;
- 1955 to 1956, 1962 to 1963—Guggenheim Fellow;
- 1959 to 1964—Treasurer, American Society of Biological Chemists;
- 1963—Visiting Professor, Institut de Biologie, Paris, France;
- 1965 to 1976—Secretary, Commission on Biochemical Nomenclature, International Union of Pure & Applied Chemists/International Union of Biochemists;
- 1965 to 1976—Director, Office of Biochemical Nomenclature, National Academy of Sciences; and
- 1966—Visiting Professor, Rockefeller University, New York, New York.

Dr. Cohn has published many times on artificial radioisotopes in biological systems; the isolation of individual fission-product species; and, especially, the development of a technique—elution chromatography on ion exchangers—that has proved invaluable in the study of nucleic acids. The techniques invented by Dr. Cohn in this field are now in use in the vast majority of biochemical and chemical laboratories and in chemical manufacture.

Recruited for the Metallurgical Laboratory (1943)

YUFFEE: My name is Michael Yuffee. I work for the Department of Energy, in the Office of Human Radiation Experiments. I'm here with Tom Fisher, also from the Office of Human Radiation Experiments. We're at the home of Waldo Cohn in Oak Ridge, Tennessee. It is January 18, 1995.

Dr. Cohn, I was wondering if we could start by going through a little bit of your background, your education, and where you're from to get us started, before we start talking about your days in the pioneering age of nuclear science.

COHN: I'm a graduate of University of California, Berkeley. I have a Master's [(M.S.) in Chemistry] and a Ph.D. in Biochemistry (1938). In 1939, I went to Harvard Medical School on a postdoctoral fellowship. I spent almost four years there, in the Huntington Memorial Laboratories of the Harvard Medical School and as a tutor in the Department of Biochemical Sciences at Harvard University. While I was there, I was solicited to join what turned out to be the Manhattan Project.[1]

I have to back up a minute and say that my Ph.D. thesis was done with artificial radioactive isotopes from [Ernest O.] Lawrence's cyclotron[2] at Berkeley. I was one of the few people in the country at that time that had experience with artificial radioactive isotopes as tracers[3] in biochemical and biological experiments. That's the reason I was sought out while I was still at Harvard and asked to join the [Manhattan] Project. They didn't tell me what the project was all about, except that my expertise was needed.

YUFFEE: What year was that?

COHN: In late 1942. In 1943, I went to Chicago and joined the Metallurgical Laboratory[4] at the University [of Chicago], which was the code name for the plutonium project under Arthur Compton. That project was to investigate the biological and biochemical hazards that might be induced by the fission products that would result from a chain reactor, if one could ever be built. Of course, this is all a prelude to producing plutonium for an atomic bomb—again, *if* it could be made. So, I was more or less in charge of what I called radiobiological aspects of the plutonium project.

YUFFEE: At this point in time, you had already been aware of the first nuclear chain reaction?[5]

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1 the U.S. Government's secret project, launched December 28, 1942 by the U.S. Army Corps of Engineers' Manhattan Engineer District, to develop the atomic bomb. Headquartered in Washington, the Manhattan Project was the Office of Scientific Research and Development Section on Uranium and was codenamed S-1 (Section One of the Office of Scientific Research and Development).

2 an accelerator in which particles move in spiral paths in a constant magnetic field

3 radioactive tags on biomolecules used to study a biological, chemical, or physical system

4 "Met Lab," the laboratory set up at the University of Chicago during World War II to lead the secret research and development of controlled nuclear fission under the Manhattan Project

5 Led by physicist Enrico Fermi, Met Lab researchers had produced the first self-sustained nuclear chain reaction on December 2, 1942. Operating initially at one-half watt, it achieved 200 watts ten days later.

COHN: I became aware of it when I joined the project in Chicago, somewhere about February 1, 1943. I was not told, when they were soliciting me to join the project, what it was all about, but when I arrived there my supervisor told me all about it. So, [in] one day, I learned the whole basis of the thing—uranium-235,[6] plutonium-239,[7] and so forth.

YUFFEE: How did you originally become interested in biochemistry and chemistry? Was it just an interest you developed while you were in school?

COHN: I drifted. I was pretty good at chemistry and physics in high school. When I went to college, my advisor there saw four years of French on my [high school] report card [and] said, "You will major in French!" But, when I went home, my father said, "You march right back there and tell them that you will major in Chemistry!" So I became a chemist more [or] less by drifting.

FISHER: Who made the initial overtures to you about the joining the Met Lab?

COHN: People that I had been associated with [at] Berkeley around the cyclotron, Joseph Hamilton and John Lawrence (who was Ernest Lawrence's brother and who was a physician; maybe some others). They were the first ones [whom] the heads of the project sought out. The reason they were sought out was that the head of the whole biological and medical wing of the Manhattan Project at that time was Robert Stone, who was professor of Radiology at the University of California [Medical School]. He knew those people, and they knew of me, and they didn't want to be separated from what they were doing because they were all involved with Lawrence's part of the Manhattan Project, so they said, "Why don't we get this guy Cohn at Harvard?"

The upshot of it was that James Bryant Conant, who was the head of the Office of Scientific Research and Development,[8] wrote a letter to James Bryant Conant, President of Harvard University, asking whether Waldo Cohn, working as a postdoc at the Harvard Medical School, could be spared. My boss asked me if I could [be] spared for an important war

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6 ^{235}U accounts for just 0.7 percent of all natural uranium; it has 143 neutrons in its nucleus, compared with 146 neutrons in the more abundant uranium-238. The slight difference in atomic weight between the ^{235}U and ^{238}U isotopes figured greatly in nuclear physics during the 1930s and 1940s. Uranium-235 could fission with slow neutrons, making a chain reaction possible. What was unknown was whether it could also fission with fast neutrons in a chain-reacting manner, to allow scientists to build an atomic bomb. See F.G. Gosling, *The Manhattan Project: Making the Atomic Bomb* (DOE/HR-0096), September 1994.

7 Chemist Glenn T. Seaborg had identified plutonium in February 1941. "By May he had proven that plutonium-239 was 1.7 times as likely as uranium-235 to fission. This finding made the Fermi-Szilard experiment more important than ever as it suggested the possibility of producing large amounts of the fissionable plutonium in a uranium pile using plentiful uranium-238 and then separating it chemically. Surely this would be less expensive and simpler than building isotope-separation plants." Source: Gosling, *ibid.*

8 established by an executive order June 28, 1941—six days after German troops invaded the Soviet Union. The OSRD's director reported directly to the President and could invoke the prestige of the White House when dealing with other Federal agencies. The National Defense Research Committee, at the time headed by Harvard President James Conant, became an advisory body responsible for making research and development recommendations to the OSRD.

project, and I said, "Whatever you think." He told James Bryant Conant, President of Harvard, who wrote to James Bryant Conant, the head of OSRD, and said, "Yes, Waldo Cohn could be spared!"

FISHER: Remarkably simple process.

(laughter)

COHN: Yes, I thought that would amuse you, that [Conant] was wearing two hats.

FISHER: A lot of people wore two hats back in those days.

COHN: So, anyway, my boss didn't want me to leave, but he figured the war project was more important. I have a letter from the Harvard Board of Trustees giving me leave of absence for the war effort in 1942, but it wasn't until [February] 1943 that I actually joined.

YUFFEE: When you got to Chicago and to the Met Lab, what research did you do in terms of trying to find out biological—

COHN: —The first thing was to get our hands on some fission products, because this was going to be a laboratory investigation. I was a laboratory person; I wasn't just going around interviewing people. I had to set up a laboratory, and then, hopefully, we could get some fission products to experiment with and put them into animals and see how dangerous they were.

YUFFEE: What type of research were you doing with the animals in terms of trying to figure out biological and biochemical hazards?

COHN: This was all [only] planned, because there wasn't a going reactor. There was no plutonium; there were no fission products. This was all on paper.

The first thing I had to do was set up a laboratory and be prepared for when there would be fission products to work [with]. In the meantime, the project as a whole was devoted to building the graphite reactor at Oak Ridge[, Tennessee]. I guess it was still in the process of being built [because] it hadn't started operating yet.

The [plan] was that each one of us at Chicago would double the size of our group, from the top to the bottom, and half would go to Oak Ridge, when the reactor would be going, and half would remain at Chicago. I elected to go to Oak Ridge, where we would actually prepare the fission products and send them back to the half I would leave at Chicago, [where] they would do the biological experimentation. So, I became a nuclear chemist involved with isolating the fission products from the exposed uranium.

YUFFEE: And then you would send the products back to—

COHN: —Chicago for the biological experiments.

YUFFEE: And who was carrying those out in Chicago?

COHN: I think my second-in-command was [Ray] Finkle.

YUFFEE: Would these biological experiments be done with animals?

COHN: Yes.

YUFFEE: Were there ever any human subjects used?

COHN: No. That's what I said over the phone; that tying me in with human experimentation is misleading. It's true that some of the radioisotopes were [later] used as tracers in humans. However, there is an enormous difference between a tracer dose and a therapeutic dose—factors of a million or more.

FISHER: Can you explain that more? Can you elaborate on that fine point?

COHN: Well, you're aware of the concept of tracers; where you put a small amount of a radioactive substance in that will mimic the movements of a nonradioactive [substance]. For example, if you want to follow phosphorus in a water stream, you might want to put in radioactive phosphorus at one point, and at another point analyze the water for radioactive phosphorus. You can't just analyze [for] phosphorus, because it is always flowing [by]. [The] radioactive material you put in [is called] a tracer. It's as if you put in a red dye at one point and then look for red dye at the other point, there being no red dye in the stream that you are talking about.

The same is true with metabolic pathways in the animal body. If you want to know how sodium was behaving in the bloodstream, how fast it goes from one point to another, you would put in radioactive sodium at one point, take a sample of blood at another point, and measure how much is there and low long it took it to get there. That gives you an idea of sodium transport, or it could be phosphorus transport.

The metabolism of things in a mammalian body is very complex; we call it biochemistry. The whole science of biochemistry has profited by the use of radioactive tracers—carbon-14, tritium,[9] radioactive phosphorus, radioactive calcium, radioactive sulfur—all [enable us] to trace the metabolic routes of things that [exist] in the body. That was the kind of work I did as graduate student; that's what I envisioned I'd be doing on the project.

Isolating Fission Products at Wartime Oak Ridge

COHN: But to get back to this business of the fission products, no one had been isolating them. It wasn't even known how many there were, or what their properties were. So, I became a nuclear chemist, involved with exploring how to get the fission products so they could be used as tracers in experiments; to see how they behaved in mammals. For example, whether they congregated in a certain place; whether radioactive strontium, if we could ever get our hands on it, would localize in a certain

[9] a radioactive isotope of hydrogen having an atomic weight of three

place. We know now that it does: it localizes in bone. This was the view of things at the time.

However, my work at Oak Ridge was not concerned with animals, even though the group I left in Chicago was. We, [at Oak Ridge], were concerned with how to isolate the fission products, one from the other, so they could be used in tracer and metabolic experiments at Chicago. I became a nuclear chemist rather than a biochemist, which was my training.

FISHER: In fact, you actually headed up the Radioactive Chemistry Division. That was your title?

COHN: I was originally part of the Health Division, which had several parts. One of them was my experimental part; the other was a radiological part; another was just taking care of the health of the workers. So, I don't know what they called my group. It might have been the Radiobiological Group.

FISHER: And you worked under Kenneth Cole?

COHN: In Chicago, I worked under Cole; "KC," we called him. He's the one [who] was my [first] supervisor. When I came to Oak Ridge, Cole had sent his [counterpart] to Oak Ridge. That was Howard Curtis. So, my immediate supervisor at Oak Ridge was Howard Curtis. We were all in the Health Division, but what my group was called, I really don't know. All I know is that we started working on building a "hot lab" and learning how to isolate the fission products and separate them, and we became so successful at that (and that's in one of those documents I left for you or I'm giving to you) that [the chemists] said, "Here's this guy Cohn and his group doing all this chemistry. They belong in the Chemistry Division." So I became Group Six in the Chemistry Division. I don't know if they ever had formal titles. Group One was Seaborg's[10] group, with Pearlman. Other groups were headed by George Boyd, Milton Burton, [and Charles Coryell].

I spent most of my time at Oak Ridge during the war years in the Chemistry Division, but doing exactly what I told you I was doing: trying to isolate the individual fission products so they could be experimented with. The idea of using them in bulk [(therapy)] was not [even] in our thinking. No one had isolated these before. We became very successful at it.

YUFFEE: Which elements were you isolating?

COHN: As many as we [could] get our hands on. As many as had half-lives long enough to be worked on. Some of them have half-lives of seconds and are gone before you can even get the material out of the reactor.

YUFFEE: So, both short-lived and long-lived?

[10] Glenn T. Seaborg, U.S. chemist (born 1912); discovered plutonium in 1940 and played a key role in the discovery of more than half a dozen new elements through the 1950s

COHN: It was the long-lived ones that we were concerned with: They are the main biological hazards. The short-lived ones are gone before anybody could come into contact with them. To name just a few, there was barium, lanthanum, samarium, praseodymium, and neodymium, and all the [other] rare-earth elements. The ones that are in larger yield and of longer half-life,[11] like barium, strontium, and cesium.

YUFFEE: You would isolate them and send them up to Chicago?

COHN: We first had to try to isolate them. We did [ship] some off to Chicago, and they were used experimentally up there and [the results were] published from up there. I had no knowledge of that until later on. I was profoundly uninterested. I was only concerned with the actual preparation of the material.

YUFFEE: Were they used for tracer studies that you know of?

COHN: Yes. I believe there are several publications in the Manhattan [Engineer] District[12] [papers] from Chicago, and Finkle's name would be on some of them. The other names I've lost track of, because eventually I lost contact with the group I left in Chicago. It was exciting and interesting enough just to try and prepare these materials. This led into the idea of using the nuclear reactor to prepare other radioactive elements, nonfission products like phosphorus-32, carbon-14, etc., and that's the burden of that long document on isotopes from the Manhattan Project, the 10-page document.

FISHER: This *Science* article from 1946?[13]

COHN: Yes. So, the isolation of fission products [and] the preparation of nonfission products of potential [research] usefulness led to that catalogue.

FISHER: Do you recall what you thought, or what you think, as to basic objectives of this MED research? Do you think [MED's Division of Biology and Medicine was established] to treat *over*exposure to radiation?

COHN: Again, I go back to the fact that the use of radioactive materials [to study the effect of] radiation on human beings, or even on animals, was the farthest thing from my thinking. I had no interest in that; no part of it. I was interested in the preparation [of] radioactive isotopes for biomedical and chemical [research].

FISHER: I understand.

YUFFEE: Maybe I can take you back a little bit, since some of the people that you've mentioned have passed away and we can't get a chance to talk

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[11] the time required for half the atoms of a radioactive substance to decay

[12] the U.S. Army Corps of Engineers organization set up to develop the atomic bomb under the ultrasecret Manhattan Project. Originally headquartered in New York, it was moved to Washington, DC, and finally to Oak Ridge in the summer of 1943.

[13] P. C. Aebersold and W. E. Cohn, *Science* 103:2685 (1946), pp. 697–705.

with them. We like to ask some of the people we talk to if they were close with people like Joseph Hamilton[14] or John Lawrence,[15] to see if they can tell us a little bit about them. For example, did you work with Dr. Hamilton when you were at Berkeley?

COHN: No. I didn't work *with* any of them. We worked separately. Each of us was an independent researcher. I was working on my Ph.D. in the Biochemistry Department. He was, I think, attached the Physics Department; and what experimentation he was doing, I don't know. We had absolutely no [work] contact other than personal contact with each other; no scientific contact. We were all part of the various people of various disciplines that were hovering around Lawrence's cyclotron [to obtain] the radioactive elements that could be produced there.

Use of Cyclotron-Produced Radiophosphorus for Ph.D. Research and Cancer Therapy

YUFFEE: Did you ever use any of the elements produced from the cyclotron in your research?

COHN: Yes, my whole [Ph.D.] thesis work was done with radioactive phosphorus produced in the cyclotron. In those days, I had to wait six months for the cyclotron to prepare a sample for me. Here's a guy whose whole graduate research was waiting on the vagaries of the cyclotron, which was a pretty new and experimental thing in those days. None of this was a routine business then.

YUFFEE: Were you doing tracer studies as part of your research for you Ph.D.?

COHN: That was my *entire* research, in rats.

YUFFEE: In animals?

COHN: In rats. I was using radioactive phosphorus because phosphorus is [an] important element, biochemically. Such things as tritium [(radioactive hydrogen)] and carbon-14 hadn't even been discovered or invented in those days. Otherwise, I might have gotten to use them, too. Getting

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14 Joseph Hamilton, an M.D., worked at Crocker Laboratory, then the site of a 60-inch cyclotron that he operated to produce radioisotopes in support of research and some medical diagnosis and treatment. Crocker was part of the Lawrence Radiation Laboratory, later renamed Lawrence Berkeley Laboratory. Hamilton is discussed in several transcripts of this series, notably in the interviews with John Gofman (DOE/EH-0457, June 1995) and Earl Miller (DOE/EH-0474, June 1995). Hamilton spent most of his career at Lawrence Radiation Laboratory before dying prematurely of leukemia brought on, colleagues believe, by occupational exposure to radiation.

15 Dr. John Lawrence, brother of Ernest O. Lawrence, was Director of the Division of Medical Physics at the University of California, Berkeley. He operated a clinic at Donner Laboratory, where he treated leukemia and polycythemia vera patients with radioactive phosphorus. For a colleague's recollection of Dr. Lawrence's clinic, see in the interview with Dr. John Gofman (DOE/EH-0457, June 1995), the sections "From Research to Laboratory Production of Plutonium," "Medical Treatments With Radioactive Phosphorus (^{32}P)," "Conflict Between University of California San Francisco and Berkeley," "Heparin and Lipoprotein Research With Human Subjects," and "Radiophosphorus Therapy for Polycythemia Vera."

back to radioactive phosphorus, my first wife, whom I married in 1938, came down with a type of cancer that required an operation and then a follow-up with radiation. The radiation was administered by radioactive phosphorus. That was a "human experiment," if you like, since it was very iffy whether the radioactive phosphorus would cure the cancer or not.

YUFFEE: Was it your suggestion to use the radioactive phosphorus?

COHN: No, it was the Department of Medicine. Robert Stone, I mentioned, was the head of the Department of Medicine. Radioactive phosphorus [was in general use] for treating leukemia and other bone diseases; [it] was a burgeoning experimental approach at that time.

YUFFEE: Just out of curiosity—

COHN: —[Not at all!] As a matter of a fact, many people were being treated with radioactive phosphorus from the cyclotron.

YUFFEE: Was is by injection or total-body irradiation, and how was it administered?

COHN: No, [the] radioactive phosphorus was either injected or swallowed. Radioactive phosphorus is a beta emitter[16] and you can't use it from the outside [of the body] because [the radiation] won't get [into the blood and bone]. It will only penetrate a few millimeters, if that far, so you can't treat bones [from the outside].

YUFFEE: Is that where it localizes—in bones?

COHN: Well, bone is mostly calcium phosphate. And, of course, phosphorus is involved in other metabolic [processes], also. It tends to be deposited in bone, and that's where most of these tumors were located. That had already become a fairly standard treatment (using radioactive phosphorus) and I'm sure that's all in the medical literature from John Lawrence and other people associated with it. That kind of treatment migrated with the cyclotron. For example, MIT[17] built a cyclotron and St. Louis[18] built one. [They made] radioactive phosphorus for medical men to use in the treatment of cancers. It may have *started* in Berkeley, but it spread pretty rapidly.

YUFFEE: Do you know anything about how the isotopes that were produced in the cyclotron were [distributed] to other facilities that wanted to use them? Was there a formal process of other institutions trying to procure those isotopes?

COHN: I really don't know. All I know is that radioactive phosphorus from Lawrence's cyclotron from Berkeley was being used to treat people up and down the West Coast for cancer.

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[16] a radioactive substance that emits electrons or positrons during radioactive decay

[17] Massachusetts Institute of Technology, Cambridge, Massachusetts

[18] at Washington University

One of [those treated] was an attaché at the Soviet Consulate in San Francisco. It got one of my friends into hot water, because during a transfer of radioactive phosphorus for treatment of that person, somebody got the idea that my friend (who had prepared the phosphorus for this transfer) was a Soviet spy. They hounded him out of Berkeley and hounded him for [many] years thereafter. That's the famous Martin Kamen. You may have heard of him.

YUFFEE: That's unfortunate.

Push to Find Commercial Uses for Crocker Lab's Radioisotopes (1940s)

FISHER: You bring up a very interesting point when you talk about the very quick period during which all of this previously academic research was put to very *practical* uses in the treatment of cancer. I'm wondering how that manifested itself, from the fall of 1942 to spring of 1943, when the Manhattan Project, in a period of only six months, took all of this academic research-oriented [work] and put it into practical use with the creation of new elements on a larger scale.

COHN: Well, it's hard to answer your question, but I'll say this: the idea of using radioactive materials as tracers started in Denmark, well before [World War II]. Not the phosphorus produced by the cyclotron, but [by] radium-beryllium exposure. The basic concept was already in the field.

Now, [Ernest] Lawrence, with his cyclotron, was very anxious to get money to support not only the building of the cyclotron and its maintenance, but also to continue the running of the laboratory. Any possible use of radioactive materials, [any] *practical* use, he could use as a gimmick to convince donors to contribute money to his project.

When the idea of using radioactive phosphorus to treat leukemia[19] and other cancer diseases came about, Lawrence was all in favor of it because it was a practical application [of what] had been, up to that time, pure research. He would push it.

The radiologists at that time knew about the dangers of radiation, but the use of radiation to treat cancer was already [well-known]. It goes without saying that if you could kill the cancer cells with radiation, you could also kill normal cells, and the hope is that you would kill the cancer cells off before you did much damage to normal cells. I think the people who use radioactive material in humans were [all well] aware of the fact there is a plus and a minus; there still is. Any radiation treatment, even today, runs a risk of damaging normal tissues.

FISHER: How about the transformation, during the Manhattan Project, from the theoretical idea that they could create large quantities of nuclear materi-

[19] any of several cancers of the bone marrow characterized by an abnormal increase of white blood cells in the tissues, resulting in anemia, increased susceptibility to infection, and impaired blood clotting

als to the actual manifestation of that idea; that it could actually be done? Did that affect your work at Oak Ridge?

COHN: I would say, no. If anything, it was a foundation of my interest in doing what I did. I knew how important radioactive materials were, as tracers. I was profoundly uninterested in their use in treatment of any disease. Such things as radioactive cobalt for the cobalt "bomb"[20] was not of any interest to me. Medical people would be interested in that, but I certainly wasn't pushing it.

Calculating the Toxic Effects of Inhaled Radioisotopes (Mid-1940s)

YUFFEE: There was a specific question I wanted to ask you that I should have asked a little time further back: I noted, in looking through some of your publications, that you did some research on inhalation studies in the mid-1940s.

COHN: You're talking about a publication about the toxicity of inhaled and ingested [radioactive substances]. That was just on paper and pencil. I didn't do any work at all; it was just calculation.

YUFFEE: Based upon what type of prior research?

COHN: Just the properties of the radiation itself; the energy of alpha,[21] beta, and gamma rays[22] and their metabolic routes, once inhaled or ingested. For example, the fact that strontium and phosphorus go to bones.

YUFFEE: Do you think this work became the basis of any inhalation studies done further down the line?

COHN: I doubt if anybody paid any attention to it. It was pure theory.

YUFFEE: Well, that's too bad.

COHN: *Ingested*, of course, is another matter. That means you actually swallowed it. Inhalation could be accidental. I didn't do any work at all; it was just paper and pencil.

YUFFEE: Was a lot of your early work—

COHN: —As a matter of fact, Karl Morgan[23] did the same kind of calculations and published it six months later, using different symbols for the same things I was doing. I think he was a bit jealous that I invaded his field of Health Physics.

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[20] referring, in this context, to the use of radioactive cobalt externally applied for medical treatment, not to a nuclear weapon incorporating cobalt in its design

[21] a positively charged particle, consisting of two protons and two neutrons, emitted in radioactive decay or nuclear fission; an alpha particle is the nucleus of a helium atom.

[22] highly penetrating photons of high frequency, usually 10^{19} Hz or more, emitted by an atomic nucleus

[23] For the transcript of the interview with Morgan, see DOE/EH-0475, *Human Radiation Studies: Remembering the Early Years; Oral History of Health Physicist Karl Z. Morgan, Ph.D.* (June 1995).

YUFFEE: Actually, I just spoke with Dr. Morgan a couple of weeks ago, and he mentioned that.

COHN: Actually, we had adjoining offices in the early days at the Oak Ridge National Lab, or Clinton Lab as it was called in those days.

Oak Ridge Graphite Reactor Becomes a Postwar Source of Radioisotopes

YUFFEE: Why don't you talk a little about once the war came to an end, and how your work changed or progressed at Oak Ridge.

COHN: Well, I had become aware of the fact that the reactor does more than convert uranium-235 and create plutonium-239. I was aware of that. I was also aware of the fact that it is a neutron[24] generator, and the neutrons could be used to make other radioactive substances. For example, even Lawrence's cyclotron, which used deuterons[25] to produce ^{32}P [(phosphorus-32)] from ^{31}P, was essentially adding a neutron to ^{31}P. But, here [in the reactor,] you had a neutron generator; you don't have to have a cyclotron. Therefore, exposing things to neutrons in the reactor would produce radioactive substances by transmutation. For example, to make ^{32}P, you expose sulfur and you extract the radioactive phosphorus from the sulfur.

So, I was aware how important they were to my field of biochemistry and [to] biology, in general. Also, I was spurred by people on the outside who were aware of this fact, once the veil of secrecy was lifted in 1945,[26] and urged this course of action on the Project. I was the guy who was already making radioactive materials.

We sat down and figured out what we could make by exposing things in the neutron generator—that is, the reactor. That was the source of the [isotope] catalog that was the *Science* article; essentially sitting down and saying, "We can make this from this in such activity," all of which was in the literature, by the way.

Obviously, the administration of the Project was all in favor of this because here was a [long-term] use for the [Oak Ridge] Graphite Reactor. It was no longer of any use [for plutonium production], because Hanford was operating to make plutonium; and the Graphite Reactor made only thimblesful, against the [large amounts] we made at Hanford, Washington. So, I had plenty of encouragement from the administration to embark on the construction of that catalogue.

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24 an elementary particle found in the nucleus of most atoms and having no electrical charge

25 a positively charged particle consisting of a proton and a neutron, equivalent to the nucleus of an atom of deuterium (an isotope of hydrogen having twice the mass of ordinary hydrogen)

26 Once atomic bombs had been dropped on Hiroshima and Nagasaki, the world knew about the United States' atomic capability and there was no longer a need to maintain strict secrecy about the fission research of the previous years.

Work With Aebersold to Create the Isotopes Distribution Committee (1946)

YUFFEE: And this was for the purpose of distributing—

COHN: —making isotopes available to qualified researchers, yes. If you read the first part [of the 1946 *Science* article], the administration part, that's what Paul Aebersold[27] was responsible for. I wrote the second part—what could be made, what activity, and how much, and so forth.

YUFFEE: Did you help play any role on the administrative side?

COHN: No, except we were in close contact. I was aware of what he[, Aebersold,] wrote, and [I was] in a position to make criticisms, and he was in a position to ask me questions about the technical side, which I was in charge of.

YUFFEE: Did this lead to the formation of the Isotopes Distribution Committee?

COHN: I think it was a combined *putsch*[28] of Aebersold and myself which led to the formation of the committee. We weren't in [a] position to do anything; after all, we [were] still under hierarchical command, going right up to General Groves.[29]

YUFFEE: Was it General Groves who first pushed the notion making use of the [Oak Ridge] Graphite Reactor and distributing isotopes? Who was it who thought we should, or the AEC[30] should, distribute?

COHN: Aebersold and I did. We went up through the chain of command and they were all in favor of it, so the order came back and said "Go ahead and do it." So, I can't claim that I had the authority to do it, but I can claim I was part of the inspiration to get it done and get the authority to go ahead and do it.

YUFFEE: This may sound sort of mundane, but one of the areas where we been having trouble finding information is on the administrative workings of the committee: How did the process work when a qualified researcher wanted to procure some of the isotopes?

COHN: You mean after the system was set up?

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27 Dr. Paul Aebersold established the administrative system for distribution of radioactive isotopes. After working on the Manhattan Project at Los Alamos, New Mexico, and Oak Ridge from 1942 to 1946, he served as director of the Atomic Energy Commission's Isotopes Division at Oak Ridge from 1947 to 1957. He retired as the Director of the AEC's Office of Isotopes Development in 1965. Two-and-a-half years later, he committed suicide. For additional information on Dr. Aebersold, see "Safety of the Nuclear Industry" in the interview with Merril Eisenbud (DOE/EH-0456, May 1995); "Remembrances of Personalities" in the interview with Earl Miller (DOE/EH-0474, June 1995); and "Oak Ridge Committees (Isotope Distribution, Human Use, et al.") and "Vanderbilt University Study of Pregnant Women and Iron-59" in the interview with Karl Morgan (DOE/EH-0475, June 1995).

28 pronounced "pōōsh"—a sudden political revolt or uprising

29 General Leslie R. Groves, U.S. Army, took command of the Manhattan Engineer District in 1942 and led it to completion of the Manhattan Project.

30 the U.S. Atomic Energy Commission, predecessor agency to the U.S. Department of Energy and Nuclear Regulatory Commission (NRC); established January 1, 1947

YUFFEE: Yes.

COHN: Well, that's all set out in the first part of the *Science* article: who applies to whom and what committees have to pass on it.

YUFFEE: Did it actually follow what was set out in the guidelines?

COHN: I believe it did, but it was all in Aebersold's department. He was in charge of the [distribution]. I was in charge of the production of the material itself, the preparation, and the shipping; everything from insertion of material in the reactor to extracting it, packaging it, and preparing it for shipment.

YUFFEE: Were the same standards used?

COHN: I got my orders about what to ship to whom from him [in] the Isotopes Division of the AEC—that is, from Aebersold.

YUFFEE: Did you sit on the Isotopes Distribution Committee?

COHN: I probably sat on it and may have fallen asleep during the deliberations. It was all set up for P.R.[31] purposes, but I do think it was followed very carefully because we [were] still under military dictatorship, so to speak.

General Groves's Relations With the Scientists

FISHER: Was it very much of a "military dictatorship?"

COHN: Well, General Groves had to pass on *everything*.

FISHER: And he ruled with an iron fist?

COHN: Well, he was the boss. For example, when it came to pricing these things, Aebersold and I recommended a certain price structure. For example, for carbon-14, General Groves wanted a price on it that, essentially, amortized the construction of the reactor itself, which was a [war] project. I got him to the phone and I said, "Look, General, you put that price on carbon-14 and you won't sell a microcurie of it. It has to be priced where people can afford it and you have to write off the Graphite Reactor as part of the war project. You can't [put] it on the back of biological researchers." So we got decent prices.

FISHER: So he rolled over in that instance?

COHN: Oh yeah, he rolled over on a lot of things, like keeping Oppenheimer[32] in charge although he was convinced he was a neo-Communist. You know all about [that]. That's public information.

FISHER: Tell us something we don't know—

COHN: —Groves had to yield on a lot of things.

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[31] public relations

[32] J. Robert Oppenheimer, U.S. nuclear physicist (1904–67) who played a principal role in the development of the atomic bomb

FISHER: Can you give another example we haven't heard?

COHN: Every month we [had] to have Project meetings at the University of Chicago, during the war years and for a short time thereafter, where the leaders of the various projects (like Oak Ridge and Ames[33] and at other places). We met for a couple of days to exchange information. This was highly secret, of course.

At one of these meetings, I remember General Groves being present, and he wanted to address the assembled multitude of high-powered scientists (including me), and he said, "If you guys don't make this work, my ass is in a sling." In other words, he admitted that he was entirely dependent upon the scientists and engineers to make it work; he couldn't do it himself.

FISHER: So, he really did listen to them, as in the pricing of the radioisotopes?

COHN: There was an element of reasonableness in his dictatorship.

FISHER: You'd be surprised. It's remarkably difficult today to try and weed out all the information that's there to come up with an accurate representation of the power structure and whether it was a case of the tail wagging the dog and who really was in charge, etc.

COHN: Well, it went both ways. Colonel Nichols[34] was here in Oak Ridge and he was Groves's second-in-command. Something that might have originated with me would go out through the Laboratory power structure from the Director of the Laboratory to Colonel Nichols, and if he couldn't decide, he'd bump it up to Groves. He would decide, and his decision would come down the same chain [it had gone up]. A lot of discussions could be made right at the Laboratory, but when it came to producing materials at Government expense and selling them outside of the Laboratory, this would be a major a policy decision [that] could not be made entirely within the Laboratory.

YUFFEE: Did the system change at all when the AEC took over?

COHN: I think it must have changed, but remember that by 1947, I was out of the business altogether. I had moved to [the new] Biology Division and was doing nucleic acid[35] research.

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[33] Ames Laboratory in Iowa, a small independent laboratory spun off from the Metallurgical Laboratory in or around 1944

[34] Colonel Kenneth D. Nichols, U.S. Army, was General Groves's chief aide and troubleshooter for the Manhattan Project.

[35] any of a group of molecules, either DNA or RNA, that carry genetic information directing all cellular functions

Transfer to the Oak Ridge Biology Division (1947)

YUFFEE: That was the end of your—

COHN: —end of my involvement with radioactive isotopes, 1947.

YUFFEE: What made you decide to make that switch?

COHN: It was obvious that isotopes were going to be a major production facility operated out the Laboratory, and therefore it [would be] on a long-term basis; not this hand-to-mouth business under which we were operating until then. I was asked if I wanted to be in charge of the [Oak Ridge] Radioisotope Production Division far into the future and I said, "No, I'm a biochemist and I want to go back to biochemical research." I transferred to the Biology Division, which had been set up in 1947.

YUFFEE: Was that under Alex Hollaender?

COHN: Yeah. He said, "Why don't you come over and join us?" So, I gave up isotope production altogether.

YUFFEE: What type of work did you then do—nucleic acid studies?

FISHER: The ion-exchange[36] chromatography?[37]

COHN: I brought that from my fission-product separation [work], because our big contribution to the chemistry of fission products was introducing ion-exchange chromatography as a way of making [isotope] separations. We made the first clean separations of radioactive rare earth [elements] produced [by] fission, which was quite a chemical advance. We published that in the *Journal of the American Chemical Society*, along with 11 other papers on the same subject. You're probably aware of that.

FISHER: Can you talk a little more about the nature of that work and the role it played in your research later on?

COHN: Well, having developed ion-exchange chromatography and learned how to use it to separate materials that are difficult to separate any other way, that can't be separated by precipitation[38] or volatilization[39] or solvent extraction[40]—ion-exchange chromatography is extremely useful. It was quite a technical advance in chemistry.

When I went back to the Biology Division, I got involved with nucleic acid research, which I'd just had a trace of interest in when I was an graduate student. One of the problems of nucleic acid research was.

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[36] the process of reciprocal transfer of ions between a solution and a resin or other suitable solid

[37] a technique for identifying the components of chemical mixtures separated by preferential adsorption on an adsorbent medium

[38] the act of separating a substance in solid form from a solution

[39] the process of passing off as a vapor

[40] separation of two different chemicals by taking advantage of their differing solubilities in organic solvents

separating the various hydrolysis[41] products of nucleic acids: So, I applied ion-exchange chromatography, and that not only separated them cleanly, but even discovered dozens of new nucleotides[42] whose existence hadn't been discovered.

I became very prominent in the field of nucleic acid chemistry and published in that area for many years. In fact, I stayed in that field until I retired in 1975. In a way, I am still involved since I'm editing, for *Academic Press*, a series of volumes in this field. I didn't do any radioactive work or have anything to do with radioactive isotopes after 1947.

Adherence to Radiation Standards

FISHER: Just one quick question: Before you left the area of radioisotopes, during the time that you were involved in creating these materials down here in Oak Ridge, were you ever concerned or ever involved in any way in a program of standards that might have been established? Did that enter into your work, or your concerns, at all?

COHN: It did. We were aware of them. We were always aware of the hazards. After all, the radium-dial painter studies were back in the '20s, and my boss at Harvard was the person who discovered that, so I was aware of the effects of radiation on living matter. I was aware of it, and the whole project was aware of it, since my part of the project was under the supervision of Robert Stone, who was a Professor of Radiology, and my next-door neighbor in the Laboratory was Karl Morgan of the Health Physics Group. We were very much aware of radiation hazards and what the standards were. We had all kinds of instruments and devices that were always telling us what we were being exposed to, and those things were being monitored every day; film badges,[43] for example.

YUFFEE: Was it Karl Morgan's group [(Health Physics)] that was doing the monitoring?

COHN: Yes, that was their part of the project.

YUFFEE: Sort of just the occupational health physics aspects of it?

COHN: Instead of...?

YUFFEE: I guess, as opposed to a more research-oriented health physics aspect?

COHN: It was research, but also protection of personnel. I remember once, for example, transferring some "hot" liquid from behind the lead barrier to a container on the outside, a very small amount of liquid in a glass tube. We had to take it out of the barrier and drop it into a container. Very short exposures were involved, but one drop fell on the floor. Well, we could have stopped right there and cleaned it up, but instead we decided

[41] chemical decomposition in which a compound is split into other compounds by reacting with water

[42] any of a group of molecules that, when linked together, for the building blocks of DNA or RNA

[43] dosimeters worn routinely to measure accumulated personal exposure to radiation

to put a [lead] brick on top [of] it, which essentially kept us protected from radiation. When Karl heard about that he said, "Yes, but someone could stumble over the lead brick, so clean it up!"

One kind of [problem] introduced another. I'm just giving you an example of the interplay between health physics and my isotope work.

FISHER: Do you recall any discussion of maximum permissible doses that were permitted for your colleagues in your labs?

COHN: We were [all] aware of that. There were standards; the International Commission on Radi[ological] Protection, I think it's ICRP, or something like that, those standards were well known and we all read them. Karl Morgan was certainly aware of them. Most of those had to do with external radiation exposure, to an x-ray tube or to a gamma source [for example]. They hadn't begun to consider internal radiation. That's where Karl Morgan and myself, with our calculations, were coming into place.

FISHER: In fact, some of your calculations led to a whole new standard for determining dose, which was then applied to occupational situations.

COHN: Yes, they could. We were aware of the dangers. We took every reasonable precaution to stay out of those and didn't do things that would expose anyone to radiation.

Have you ever been to Oak Ridge? You haven't seen the Graphite Reactor, for example?

There's a picture there of one step in our operation, during the preparation of fission products. We got these irradiated uranium slugs about six inches long out of the reactor, [and] transported them behind heavy lead shielding to the top of our "hot" cubicle, where we were going to [dissolve] it and isolate the fission products. To get it from the lead container into our "hot" cell involved taking it out momentarily and transferring, which means that the person who was doing the transferring would be exposed to the radiation from the "hot" uranium.

We [constructed] a fishing pole, about 8 feet long with a grabber at one end and a handle at the other. Paul Shallert practiced [the transfer,] and doing it in less than eight seconds. We figured that eight seconds would give him the maximum permissible dose for that day. His instructions were that if he couldn't make it in eight seconds, [to] drop it and run: somebody else would come and try to do it.

We were wary of these things. We tried to make the calculations to stay out the danger range, or within the permissible range, as it was known at that time.

FISHER: But, point in fact, you probably wouldn't have let those recommendations stand in the way of your work personally?

COHN: We would have figured out some way to get around it, by having someone else try it and see if he could do it [in] eight seconds.

FISHER: I think that those stories and anecdotes are fun. That's an example of stuff that we cannot get from reading Newell Stannard's[44] books; he doesn't talk about that kind of stuff.

COHN: You're getting down to the personal interaction level.

FISHER: Which is the benefit, the real benefit, to readers of this transcript later on.

COHN: There are various pictures on the second floor of the Graphite Reactor building showing my group in operation making radioisotopes. Every one of them is wearing a film badge. The person doing the transfer—there's a picture of him mimicking the transfer—he's wearing a [dosimeter] ring on his hand. That would be the closest thing to the source, even though it was at the end of an eight-foot pole.

There were monitors hanging around in all buildings in case anything got loose that we didn't know about; they would sound an alarm so everybody would run out of the building until they found out where the trouble was and there could be some kind of cleanup.

YUFFEE: In our interview with Dr. Morgan, he mentioned uranium slugs and he said that he noticed, when he first got to Oak Ridge, that the workers were carrying them without their lead gloves because they found the lead gloves too cumbersome to work with. He found that to be a concern.

COHN: Those were slugs *before* they went into the reactor. After they came out of the reactor, nobody handled those things, except with tongs, and [from] eight feet off, and one at a time. He was talking about the material going *into* the reactor. Those uranium slugs were canned in aluminum jackets. The only radiation coming out of [uranium] radiation is alpha radiation,[45] which won't penetrate the dead skin on your hand, let alone the aluminum jackets. So, it's perfectly safe for a worker to pick up an aluminum-jacketed slug and put it on a tray and push into the reactor.

There's no radiation to get to and there's no radiation on the aluminum slug. The radiation is all in the uranium on the inside. Further, most of the radiation from the uranium doesn't even get out of the uranium itself. It has to penetrate uranium to get to the outside. Some of the radiation that originates from the outer edge of uranium could get out of the uranium, but *then* it would have to pass through the aluminum jacket, [which it can't do].

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[44] J. Newell Stannard, a professor of Radiation Biology and related subjects at the University of Rochester (Rochester, New York)

[45] Uranium also emits beta particles because its beta-emitting decay products are present.

Self-Experimentation With Radioactive Tracers

FISHER: I have another question to ask you: Did you ever, during the course of your tracer studies, use yourself as a subject?

COHN: Out in Berkeley, I did. For example, we learned how to make radioactive sodium, and we were interested in how fast blood flows from this arm *(holding out one arm)* [to] get to this arm *(holding out the other arm)*. We would inject ourselves with a little bit (a tracer amount) of radioactive sodium and then hold onto a Geiger counter and count 1, 2, 3, 4, 5, tick, tick—it's over there [in the second arm]; so you see how fast the blood is migrating. That's an example of [a] self-inflicted radiation tracer experiment.

I emphasize the word "tracer" because these amounts are infinitesimally small. We were quite willing, though, to take whatever chances there were.

YUFFEE: So, would you say this was fairly common practice among researchers, especially at Berkeley, to use yourselves in tracer studies?

COHN: Well, it was [a] practice among researchers everywhere, actually. The tracer doses are almost definable as ones that will not induce any untoward physiological effects.

FISHER: Well, there's certainly—

COHN: —For example, the difference between radioactive iodine to *diagnose* a thyroid[46] problem [and] radioactive iodine to *treat* a thyroid problem. The dose range is over a million; you might use a fraction of a microcurie[47] for a tracer experiment, but you use maybe a millicurie[48] or more for actual treatment of thyroid [cancer]. [Also,] there are repair mechanisms for radiation damage, and a very small amount of radiation might do a little bit of damage, but it's repairable, whereas if you override the threshold, you could get into trouble.

FISHER: Do you think that's what happened to Joseph Hamilton? There are some very widely known stories about that he would use himself as a subject in his classes, practices which may have led to his early death.

COHN: That's all supposition, about them leading to an early death. I wouldn't be surprised if he did, for example, swallow some radioactive sodium or [radioactive] sodium chloride, table salt, for a class and have his hand on a Geiger counter here and let the students see how long it took for the ingested dose of sodium chloride to actually get into the bloodstream and be circulating. I mean, I can imagine him doing that kind of experiment, and I would have done it myself if I had been in a position to need that kind of experiment.

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46 an endocrine gland located at the base of the neck and secreting two hormones that regulate the rates of metabolism, growth, and development

47 a millionth of a curie

48 a thousandth of a curie; one thousand microcuries

FISHER: Well, those are exactly the sorts of experiments that I have read about Dr. Hamilton [doing]. I'm not sure what substances he ingested, but I've read about him doing these repeatedly in public demonstrations.

COHN: It probably involved radioactive sodium, because sodium has about a 15-hour half-life. So, in about a few days, it's all gone. And, it's a gamma emitter: it doesn't localize any particular place.

As a matter of fact, my ex-wife died, in 1941 I believe it was, and I remember once when she ingested some radioactive sodium as part of one of Hamilton's experiments. [Just] after that she came [down] from the Radiation Lab, where this had taken place, down to my lab, where I was trying to measure radioactive phosphorus on my Lawritzen electroscope.[49] The minute she walked into the room, the background of my Lawritzen electroscope went up. From 15 feet away, that's where the gamma radiation was affecting my electroscope. A very tiny amount of it was enough to influence my counting mechanism.

FISHER: Well, was your wife a part of a cohort that Dr. Hamilton was studying, or was she just a volunteer, as a friend?

COHN: She was like myself: she was working for a Ph.D. in the same department at the same time I was. At that time, we weren't married.

Researcher Knowledge of Radiological Hazard and Informed Consent

YUFFEE: One question that you may have thoughts on, or you may not, is that with tracer studies, the biological hazards from them are close to nil. In your mind, is there a philosophical difference to be made about a tracer study done on a willing or knowing patient versus somebody who doesn't know that they're having a tracer administered to them?

COHN: I really can't answer that question. At the time that Hahn at Nashville[50] was doing his experiments, he certainly believed that the amounts of radioactive iron he was using (fractions of a microcurie) were innocuous. Whether he discussed this with the subject[s] or not, I have no way of knowing. But certainly, he was well aware of all the hazards of radiation; he's among the people [who] wrote the standards.

YUFFEE: Who's this?

COHN: Paul Hahn, at Vanderbilt [University in Nashville]. He's long since dead, but he [(meaning his work)] and Vanderbilt are part of class-action law suit involving radioactive iron immediately after the war years. It's a roundabout way of answering your question. The people doing these experiments were well-aware of whatever was known at that time about

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[49] a device for detecting the presence and determining the sign of electric charges by means of electrostatic attraction and repulsion, often between two pieces of gold leaf enclosed in a glass-walled chamber

[50] Dr. Paul Hahn at Vanderbilt University performed research with radioactive iron and pregnant women. See "Vanderbilt University Study of Pregnant Women and Iron-59" in DOE/EH-0475, *Human Radiation Studies: Remembering the Early Years; Oral History of Health Physicist Karl Z. Morgan, Ph.D.* (June 1995).

hazards. Whether they would discuss them with the subject[s]—I use the word subject, rather than patient—I can't answer.

FISHER: Since we're talking about stuff going on at Berkeley, I once read an old Met Lab monthly report that talks about a visit you made to Dr. Hamilton at Berkeley to assist in a model experiment on gaseous fission products. I was wondering if you could talk about the internal-emitter toxicology[51] program in general at Berkeley.

COHN: I have no knowledge of it at all. If I did have the knowledge, it's long since vanished from my memory, because I can't remember such a visit or such a consultation. I did make two or three trips to Berkeley from Chicago right after I joined the project to find out what was going on there that might influence my work on radiotoxicology, but I don't remember any work of that kind there or my involvement in it.

FISHER: Fair enough.

Research on Nucleic Acids

YUFFEE: Maybe you could tell us a little bit more about the nucleic acid studies that you did.

COHN: Well, they have no relevance at all to radiation.

YUFFEE: Yes, but it'll give a well-rounded view of your work.

COHN: Well, when I came into it in 1947 I wanted to investigate the turnover, the half-life, of nucleic acids. In order to do that I needed to separate the four nucleotides (the adenylic, guanylic, cytidylic, and uridylic acids), of which RNA[52] is composed. The idea was that I would inject an animal with ^{32}P, isolate the nucleic acids, and then isolate the four nucleotides and see how radioactive each one of them [had become]. It's rather an elementary, stupid experiment, but it was one way of getting started.

I never got around to doing those experiments, because I got more interested in the chemistry of separating the four nucleotides, which never [had] been done before in any reasonable way. So, I applied ion-exchange chromatography and [thereby] found out that they could all be separated very simply.

But in the meantime I found out there were other nucleotides there that we hadn't been aware of. So, one thing led to another, and I found myself knee-deep in investigating the basic chemistry of RNA rather than the turnover that I had in my mind. That technique of using ion-exchange chromatography to separate the nucleotides permeated the whole field of biochemistry and was applied to many other things: separation of sugars and sugar-phosphates, and other biochemicals. Column chromatography, as it's now known—

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51 the branch of pharmacology dealing with the effects, antidotes, detection, etc. of poisons

52 ribonucleic acid, associated with control of chemical activities in cells

YUFFEE: —Chromo chromatography—

COHN: *Column*chromatography, the idea of flowing things down a column in such a way that they emerge at different [times at] the bottom; you get the first one, and then another, and then another.

YUFFEE: Is this through a gel or what type of—

COHN: —The material in the column is a sort of gel, yes. Actually, the whole term *chromatography*, *chrome* from color, goes clear back to work done in Poland even before the turn of the century. I forget his name right now; [the researcher] was flowing a mixture of plant pigments down a column of starch and finding out that by washing that stuff through the column the different pigments appeared at different times at the bottom. So, just [add] the word *chromo-* for color, to *-graphy*, meaning "separation of." So anything where you flow things down [a] column as a mode of separation is known as chromatography, even though [visible] color may no longer [be] a part of it.

YUFFEE: Do you think that the early work you did in trying to separate the nucleic acids—I'm familiar with research done at Brookhaven[53] and maybe other places, where they were eventually able to tag specific nucleotides or nucleic acids and then use that.

COHN: It's become very common. For example, in the O.J. Simpson case,[54] what do you think all of those [nucleotide] bands [in the DNA-comparison charts shown by the prosecution]—

YUFFEE: —The DNA[55]—

COHN: —They are all labeled with radioactive tracers. The bands are radioactive. That's part of the DNA "fingerprint" technique. You fractionate[56] the thing and put a label on each [fraction], and then you separate them, and then you can detect each one is by its label because the amount of material is infinitely small.

So, I think that the work on column chromatography led, through one change or another, into that sort of work and into all the chromatography work in all kinds of fields in biochemistry, as a general separation technique. When I visited Osaka, Japan, back in the 1960s, my work was already well-known and was being used: that is, the chromatography of nucleotides. They took me to an observation room in the administration

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53 Brookhaven National Laboratory, Upton, NY

54 In June 1994, the ex-wife of actor and former football player and sports commentator, O.J. Simpson, and a visitor to her home were murdered in Los Angeles, CA. Mr. Simpson was charged with both murders and placed on trial. DNA from blood samples taken at the crime scene and provided by Mr. Simpson were presented as critical evidence in the trial. At this writing (June 1995) no verdict has been rendered and the trial continues.

55 deoxyribonucleic acid—a type of nucleic acid, particularly found in cell nuclei, that is the basis for heredity in many organisms. DNA molecules are constructed of a double helix held together by hydrogen bonds.

56 separate or divide into component parts

building and showed a view of what look like a tank farm: huge cylinders connected with pipes. They asked me, "What do you think those are?" And I said, "It looks like an oil refinery to me." They laughed and said, "Those are your ion-exchange columns." They were preparing nucleotides [on a massive scale] by the column techniques I pioneered some 10 years earlier.

YUFFEE: That must be gratifying to see it in a—

COHN: —so it could be used in a preparative way. Some of those nucleotides are used as flavors and other things like that. It's a big business in commercial chemicals.

YUFFEE: Did you patent any of this work? Is that something you could have done?

COHN: The column chromatography was never patented. It was all published and everybody used it. I did patent the method of making ^{32}P from sulfur in the reactor, at the urging of the attorneys. Otherwise, I wouldn't ever have thought about it. I do have one patent; not hundreds like my colleagues [may] have.

YUFFEE: But it's interesting to note how much was being done, and how many advances were being made and funded by the Government, and how a lot of them went unpatented so that they could be used generally by everyone.

COHN: It became part of the whole scientific enterprise. There was some resistance to allowing us to publish at the very beginning. I remember my first paper on nucleic acids was held up by the [security] classification office; they saw the word *nucleic* and figured it had to with *atomic* nuclei rather than *cell* nuclei. After explaining to them, I was allowed to publish.

The patent on ^{32}P was held up for years because Groves's advisors said that from it, a knowledgeable person could calculate the power of the reactor—which was all nonsense, because it was only a little experimental reactor, the Graphite Reactor here. This indicates that the wartime secrecy thing had a quite a bit of aftershock.

Getting back to nucleic acids. My application of ion-exchange chromatography to the problem I told you about, leading to the isolation of different nucleotides that had not been discovered, led rather directly to the discovery of what is now known as messenger RNA, which is the way information gets from the DNA to the protein that it specifies.

The messenger RNA had been a theoretical concept until my colleague Vullein, [whose office was] next-door to mine, applied both column chromatography and my radioactive phosphorus to the problem of how viral RNA influences the transformation of [the cell] into making more virus. So, by application of two things in which I was involved (radioactive phosphorus and column chromatography), he was able to actually show that viral RNA is the long-sought-after messenger RNA, or that messenger RNA is that substance which is produced directly from the DNA and carries a message into the cytoplasm.

YUFFEE: Takes me back to biology in high school.

COHN: I hope what I said is clear because—

YUFFEE: —Oh, definitely.

FISHER: As long as you bring it up, I do have one question I might ask you: Could you please spell the acids that make up RNA for us because it will be very tough for me to look that up when we edit this transcript.

COHN: Yeah, you want it?

FISHER: Sure.

COHN: A-D-E-N-Y-L-I-C, G-U-A-N-Y-L-I-C, C-Y-T-I-D-Y-L-I-C, U-R-I-D-Y-L-I-C.

FISHER: You saved us hours of research on those alone, I can assure you.

COHN: To be technically correct, each is followed by a space and then "acid."

YUFFEE: And these are components of the nucleotides—?

COHN: They are the components of [ribonucleic acid] (RNA).

YUFFEE: Such as—

COHN: —which is composed of those four nucleotides in various arrays. The order in which they appear in a nucleic acid in triplets are what specifies amino acids going into protein. Each triplet of three nucleotides specifies one amino acid. For example, A-A-A in a row will specify one particular amino acid. Since there are 20 amino acids in about 60 combinations of triplets, you can see why I don't have them all in my head.

YUFFEE: *(smiling)* We'll forgive you.

FISHER: *(smiling)* That certainly is complicated and over *my* head.

COHN: The whole idea of the information-transfer DNA is like RNA; it's composed of the same four nucleotides except that the uridylic is now a thymadylic, but that's a minor point. If you arrange those in triplets—one, two, three, four, five, six, seven, eight, and nine—[then] one, two, three, will be one triplet; four, five, six, another; seven, eight, nine will be another, and so on. Each one of those triplets will specify one amino acid in your final protein. Each one of those triplets is transferred to RNA, (that's the messenger RNA), which will carry that message from the DNA to the cytoplasm[57] so the protein can be made.

That's your information transfer chain, and the nucleotide sequence is the basis of all of it. All of those bands you see on the TV from O.J. Simpson's DNA test are strings of nucleotides: they may be 7, 8, or 9, or 10, or 12, or 14 [nucleotides] long, but each one will be a different sequence, and that's why they migrate to different levels. Then they're detected by the radioactive tracer on them, and the distance that they move tells you how many nucleotides there are in each one.

[57] the cell substance between the cell membrane and the nucleus

YUFFEE: Did you say what you figure that the tracer was? You know what tracer they would probably use?

COHN: Probably phosphorus[-32], because it has a long enough half-life, maybe two weeks, to stick around for a long time. [If] they used something that is short-lived, like sodium (15 hours), in two days, you would be out of stuff: it would be dead.

FISHER: So, the next question is, do you think [he] did it?

COHN: My intuition says yes, but I admit that proving beyond a reasonable doubt will be pretty hard since there were no witnesses.

Use of Radioactive Isotopes Assessed in Context

FISHER: Michael is the lawyer and we'll leave that to his colleagues. I do have another question for you about the establishment of radioisotope policy: Are you satisfied with the way this turned out in later years, after you left the area responsible for the manufacture of radioisotopes and [after] you constructed this [distribution] policy with Aebersold? Are you satisfied that this policy met the expectations of those people involved and that it was [a] productive and positive way for the effective distribution of these radioisotopes?

COHN: I think that the whole [business] was an enormous boon to human welfare. Oh yes, I'm extremely positive about the whole thing. All this ruckus that has been raised now, that these tracer doses are somehow "radiation experiments on humans," I think is quite false; in fact, [it is] beside the point.

I'm still mad at [Secretary of Energy] Hazel O'Leary for releasing all those documents without explaining that some [involved] tracer amounts, which are *not* radiation experiments on humans. It should be completely separated from experiments on humans, like the blasts out in Nevada, or Utah, or wherever they were. Also, I think she should have distinguished [them from] cancer treatments, which had a long history. Those are radiation treatments of humans, but they're not experiments in any sense of the word.

YUFFEE: Would you include in that category the TBI [(total-body irradiation)] studies that were done at ORINS,[58] that have gotten pretty wide publicity?

COHN: My impression of those is that they were all cancer treatments.

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[58] Oak Ridge Institute of Nuclear Studies, established in 1946 by the Manhattan Engineer District and operated under a Manhattan Project (and later Atomic Energy Commission) contract. The responsibility of ORINS was to train physicians and researchers in the safe handling of radioisotopes and to develop isotope applications in medicine. Today, the educational and training functions of ORINS are carried out by its successor, Oak Ridge Institute for Science and Education (ORISE).

Vanderbilt University Studies of Pregnant Women With Radioactive Iron (1945–49)

FISHER: How about the studies at Vanderbilt?

COHN: What studies, the radioactive iron?

FISHER: Well, the pregnant women studies.[59]

COHN: That was radioactive iron as a *tracer*, using a tracer dose. I've been privileged to see one of the reprints of one of the papers that Hahn published, and he says that 100,000 counts per minute [were administered]. That's a minuscule dose; that isn't even a microcurie.

FISHER: And whose paper was that?

COHN: Paul Hahn. It's his experiments that were the so-called Vanderbilt Experiments.

FISHER: Do you think that because they occurred on pregnant women, they have become a "sexy" issue for the news media and really have no scientific validity or cause for concern?

COHN: I would throw the case out of court, but then, [the] judge isn't a scientist. I think the class-action suit is simply a way of using this as a [way] to get some damages out of the Government. I think it's a nuisance suit, too: I don't believe any damage was done. Now, whether or not informed consent was given, I don't know. At the time, there was no reason to give informed consent because it was commonly believed that [that] amount of radiation would not be toxic or dangerous. But whether or not an informed consent was asked for, or given, I have no way of knowing. My whole knowledge of this has just come about in the last two or three months, when I have been besieged by both parties to the suit for affidavits on [the production of] radioactive iron.

YUFFEE: Is this [the] first time you've been asked to be involved in a litigation?

COHN: Yes, and all because of that [1946] *Science* article [by Hahn] that says that we would supply radioactive iron under those conditions and so forth and so on. But again, reading the Paul Hahn article that they gave me a reprint of, it says he got his radioactive iron from a (d, p) reaction, which is "deuteron in and protein out." That's a cyclotron product; that's not a reactor product.

YUFFEE: Would that have been a cyclotron at Vanderbilt, or would that have been one near Oak Ridge?

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[59] From 1945 through 1949, Vanderbilt University Hospital conducted studies on iron absorption in pregnant women. Participants in the study were part of a larger nutrition survey conducted by the hospital. In all, 829 normal, healthy, pregnant women ingested radioactive iron-59 in an amount ranging from 1.8 to 120 milligrams. The iron-59 was administered at various times in the gestation period ranging from fewer than 10 to more than 35 weeks. The study showed that iron uptake is related to both dosage level and gestation period. For a more complete discussion and a list of references, see "OT-11: Iron Metabolism in Human Pregnancy as Studied with Iron-59," in *Human Radiation Experiments Associated with the U.S. Department of Energy and Its Predecessors* (210+ pages), DOE/EH-0491, July 1995.

COHN: There was not one at Oak Ridge that was supplying radioactive iron. We would [make] radioactive iron by [an] N-gamma reaction in the reactor, but Paul Hahn said it was produced by the (d, p) reaction. This indicates the iron came from a cyclotron, and Oak Ridge didn't have a going cyclotron, nor was it supplying any radioactive products other than the reactor products.

YUFFEE: There was someone whose name I came across: C.W. Shepherd—

COHN: —He worked with Hahn, and then he came and worked with the Biology Division—same division I moved into—in 1947.

YUFFEE: He was with Hahn through the studies. He would have also procured the radioactive iron from a cyclotron?

COHN: I don't know whether he was with Hahn on that study or not, because his name would have appeared as an author.[60] I think he may have left. He *was* with him during the war years. I don't think he could have been [involved in the study] because—I think those experiments were done in 1946 or 1947, and Shepherd was in the Biology Division in 1947. So, I just don't know.

No Human Subjects Used by Oak Ridge Biology Division

YUFFEE: Do you know of any studies using human subjects done by the Biology Division or the Health Division?

COHN: None were ever done by [the] Biology Division, period.

YUFFEE: Karl Morgan said the same; he didn't think Alex Hollaender would have done that.

COHN: The ones done by the Oak Ridge Associated Universities, or ORINS, as it was known in those days, I am convinced, were all cancer studies and had nothing to do with experiments, as such.

YUFFEE: These would be the studies beginning with Dr. Gould [Andrews] and then Lushbaugh?[61]

COHN: Yeah, they're all medical people.

FISHER: How about Hamilton at Berkeley?

COHN: I have no way of knowing. I left there in 1938. I had limited contacted with them. We were not colleagues in the sense of ever working to-

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[60] Shepherd and Hahn did collaborate for that study, according to Karl Morgan in his interview (DOE/EH-0475).

[61] Gould Andrews directed the total-body-irradiation facilities at Oak Ridge; Clarence Lushbaugh directed the Low-Exposure-Rate Total Body Irradiator (LETBI) facility. For contrasting views on the medical ethics of those studies, see DOE/EH-0475, *Human Radiation Studies: Remembering the Early Years; Oral History of Health Physicist Karl Z. Morgan, Ph.D.* (June 1995) and DOE/EH-0453, *Human Radiation Studies: Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

gether. [Our relationship] was part of a loose array of biologically-minded people around the cyclotron.

FISHER: But, it was still a pretty closed community, especially during the war years, don't you think? Or am I wrong in that perception?

COHN: I left [Berkeley] before we got into the war. I left there in 1939. As a matter of fact, I was on my way to Harvard when Germany started World War II. I heard [about it] over the radio while I was en route.

FISHER: You didn't remain aware of the work that other individuals, even across the country, were doing?

COHN: No. Again, I didn't join the project until late 1942 and early 1943. There is quite a hiatus there.

I warned you over the phone that I had nothing to do [with] human radiation experiments.

FISHER: That's fine, but it doesn't negate the value we glean from sitting here with you this morning.

YUFFEE: In fact, the information on the creation of the Isotopes Distribution Committee and whole radioisotopes distribution is very useful, and that was one of the reasons—

COHN: —I regard that as an unmitigated good; in other words, it's triple A-plus, ranked number one.

Creating the Oak Ridge Symphony Orchestra (1944)

FISHER: And it's the thing you're most proud of in your work?

COHN: It's one of three things, certainly. Chromatography is one, because that's permeated the whole field of biomedicine and biology and biochemistry and whatnot. The work on nucleic acids which came out [of] that and led to the discovery of messenger RNA and a way of analyzing nucleic acids and—I'm going to throw you a real hooker.

FISHER: Fire away.

COHN: The creation of the Oak Ridge Symphony Orchestra.

YUFFEE: I was going to ask you about that, actually.

COHN: We just celebrated [its] 50th anniversary.

YUFFEE: One of the questions I wanted to ask you about that was, when you started it in 1944, under what was still the strict veil of secrecy—Oak Ridge being a secret town—was it because you were in a closed community where there were other musicians like yourself?

COHN: Well, if you have a few minutes I can give you the story.

YUFFEE: Yeah, please.

FISHER: *(smiling)* I'm secretly hoping asking for actually more than that, but maybe not on tape.

COHN: You can put on it tape if you like; it's also summarized on one those *C.V.*'s that I gave you.

FISHER: *(smiling)* I'm hoping for a short recital, maybe, later.

COHN: I moved from Chicago [(the Met Lab)] to Oak Ridge on September 30, 1943. I didn't bring my wife and the child I had gotten with my first wife (who had since died) with me because there was no place to live yet: they were still building the houses there. I did bring *one* thing with me, besides my extra shirt, and that is the cello you see out in the music room here. I transported that first. Left my wife behind.

YUFFEE: *(smiling)* Does that mean that the cello was your first love?

COHN: Since I was 11 years old. I was 33 years old when I moved to Oak Ridge, and I always played mostly quartet music, occasionally [orchestra] music if I had to. I have been immersed in the practical side of music all my life.

So anyway, after I got a place to live and started to work at the Laboratory, I started looking around for people to play with. First, I met a violinist, and then another violinist, and [soon there was] a group of us who were interested in [making] music. [We] met at the high school.

There were eight wind players and three string players, and I said, "This is no way to form an orchestra; you need lots of strings and very few winds. So, you guys with winds go form a band, and I'll take the two fiddlers and see if we can get enough for a string quartet," which was what I wanted. First thing, we found one, and then another.

I had some simple string-orchestra music. We started to meet at my house and play string-orchestra music, just for fun. And then, the word got around, and the first thing you know, there were more and more string players showing up: "I played in college," or "I played in high school and I like to play," [they would say]. Soon, there were more people than could be accommodated in my living room, which was in a house smaller than this.

Finally, we petitioned for a school room in one of the schools, got it, and then we [were] all spread out and we couldn't stay together [in tempo etc.] at all, and [the orchestra members] said, "You're the organizer, so you have to be the conductor." I stood up and I became a conductor. I had enough orchestra experience to know what conductors do, so the first thing I knew I had a 20-piece string orchestra.

Then, the various wind players heard about this and they said, "Look, we'd like to get in on this, too. Why can't we make it a full orchestra?" I said, "Okay, we 20 string players will *first* give a little concert to get that out of our system, and then we'll add the winds and make it a full orchestra." In June 1944, we had a string orchestra concert. Remember, this all occurred [from] September [1943] to June 1944. We added the

winds in November 1944, and I had a full symphony. We gave a symphony concert—

YUFFEE: —Oh, that's great!

COHN: —playing legitimate music. I mean, we played Schubert and Mozart; pretty badly, I would say.

These were 100 percent amateurs. Some of them did shift work.[62] We couldn't get [full] rehearsals because [some] worked at night and transportation was tough because there was gas rationing [and the] streets weren't paved; even the main street was still just gravel.

YUFFEE: Did you have difficulty filling in specific instruments; did you have trouble finding a bass player?

COHN: The funny part is, I had no trouble for the first concert. Actually had two bassoon players. The first bassoonist was named Auguste Schmidt. What a good name for a bassoon player! I have a picture of him and Oriel Snyder. The second concert, I had one bassoon; the third concert, I had no bassoons. I became expert at re-scoring. I re-scored bassoon parts into French horns, of which I had many, and into clarinets, of which I had many. Anyway, that's where it all started.

When the end of the war came, people started leaving, and I suddenly had half of an orchestra left. That's when we started [to] bring in professionals from Knoxville, to fill in holes, and it's been that way ever since. It's been basically a volunteer orchestra, with various holes in critical places filled by professionals, mostly from Knoxville. Right now, it's about 50/50, but most of the [old] amateurs are dying out. [There are] only four of us left from the full 50 years. There aren't so many new volunteers coming into Oak Ridge, because the crowd of people that's been coming in the last few decades has been mostly business people and engineers, and not college-trained musicians.

FISHER: Any names that we would recognized from the early years?

COHN: As far as players, you wouldn't recognize any. As far as soloists with us, you would recognize a few names. In the first season, we had someone name Menuhin; not Yehudi but his youngest sister, Yaltah, who was the baby of the family *and* the one the family decided *shouldn't* be a musician. They didn't push her like they pushed Yehudi. Her Army husband happened to be stationed in Knoxville, and I had known the Menuhin family since my California days, because I'm a native Californian, by the way—not just college—so I got her as a soloist.

We got Albert Spaulding as a soloist. I got Isaac Stern, in 1948, on the basis of an old friendship. There are probably some others that you may or may not have known.

[62] work on the swing shift (about 4:00 p.m. to midnight) or graveyard shift (about midnight to 8:00 a.m.)

FISHER: Any of the scientists that we spoke about this morning who may have been involved? Were any [of them] musicians in their own right, as well?

COHN: Well, I spoke [about Paul Shallert, the] guy [with] the eight-foot pole[63]—he was my first horn. And, you spoke about a double-bass player. We had one fellow who moved into Oak Ridge, in a little tiny house, with his double-bass. (He recently retired as a professor of Biology at Washington University of St. Louis.)

I have a marvelous picture taken of the Oak Ridge Symphony in rehearsal in its first or second year, when the Army was still here, and we had a lot of GIs[64] in the orchestra. It's a shot taken down [at] the first violin section, in front of the violas and looking through the conductor, so you see the bowing arm of all the first violins going down, second stand, third stand. The right arm of the first stand, concert master, has three stripes; the right arm of the second stand has two stripes; the right arm of third stand, one stripe.

YUFFEE: Was that because of the ability or…?

COHN: It was strictly ability—the real pecking order.

YUFFEE: Did you ever conduct—

COHN: —[Yes, for 11 years]. I lost most of those [musicians] when the war ended.

The three-stripe sergeant, I heard him once at a rehearsal break fiddling around [with the] Mendelssohn Violin Concerto. I said, "Can you play that?" He said, "Sure: I know it by heart." I say, "Can you play it at a concert?" He said, "Sure: you want it, we play it." So, at the second concert I had a sergeant playing the Mendelssohn Violin Concerto.

YUFFEE: Did you ever end up composing anything?

COHN: Did I ever compose anything? No; I can't even play the piano.

YUFFEE: So, basically you just—

COHN: —I played the cello and I was a conductor until I retired after 11 years. We [have gone] through a series of professional conductors since then. We're on our 11th or 12th right now.

YUFFEE: Obviously, you are still active in it?

COHN: I'm still active in it, but my days are numbered because I lost part of the use of my left arm, which is my fingering arm. I can still get up on the neck [of the cello] and finger [the strings], but I can't get into thumb position because I can't raise this part of my arm. *(points to his upper arm)* I've had various neuromuscular problems, so I think my days [as

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63 the "fishing" pole that was used to transfer uranium slugs from the reactor

64 members of the U.S. armed forces, especially soldiers

a cellist] may be numbered. I still have a 1750 cello in the other room that I play once in a while, and I get [to] rehearsals.

YUFFEE: It looks beautiful.

COHN: You didn't see it.

YUFFEE: I saw the case.

COHN: The cello is sitting out; it was built in 1750. It's worth as much as our grand piano is worth.

YUFFEE: It goes back quite a ways.

FISHER: I didn't realize that instruments lasted that long.

COHN: At the time, the dealer in Paris, where I [studied] for six months, offered me the chance to buy one built 100 years earlier, in 1650. I liked the sound of this one better; it was also less expensive. So I took the 1750 one.

YUFFEE: Seeing it's 1995, I don't think anyone would fault you.

I guess the last question, in closing unless, Fisher, you have any other questions—

FISHER: I was just going to ask a summary-type question.

YUFFEE: Basically, are there any questions that we *didn't* ask you, that we *should* have asked you?

COHN: I don't think so. I think I ad-libbed some answers to unanswered questions already—mainly, what I think of the charge of human radiation experiments.

Nuclear Energy Policy and Public Opinion

FISHER: Is there anything else you would like to offer, besides those ad-libs and your comment about Hazel O'Leary, which I swear we will not edit out of the transcript? Is there anything else you'd like to say? This is your opportunity to run the show. We offered up some verbal orchids, but we really wanted you speak about issues that you thought were important and to fill in the gaps in the historical record that publications don't always [fill].

COHN: You asked me about what I consider my most important contributions. I think I mentioned my founding of the orchestra, which is out of the loop, so to speak. But, concerning the nuclear power question, I think the public antipathy that has been raised by certain well-meaning, but misguided people—[the assertion] that there's no reasonable way of handling nuclear waste—is totally off the mark. Waste *can* be handled in such a way that it will not enter the biosphere[65] or harm anybody. Nuclear power is the answer to both the problem of global warming and

[65] the part of the earth's crust, waters, and atmosphere that supports life

fossil fuels. I'm a firm believer in nuclear power. I think it has been given a bad rap by people who ought to know better. In other words, I think the waste problem is a technical problem turned into a social problem, which it shouldn't have been. I'm halfway through this book *(handing Fisher a book)*; are you familiar with it?

FISHER: *First Nuclear Era* by Weinberg. No, I'm not.

COHN: Alvin Weinberg was the Director of Oak Ridge National Lab for [about 20] years. He's been retired for over 10 years. This [is] in his autobiography [of his career] at the Laboratory, along with his general feelings about the whole nuclear enterprise: how it developed and where it's going. The thesis to this book is that we're ending the first nuclear era but there's a second one as soon as people come to their senses and realize that there are safe nuclear reactors and that safe disposal of nuclear waste is feasible.

YUFFEE: One of the issues seems to be that there may be the capability to dispose of nuclear waste safely, but that people aren't willing to commit the funds necessary and that's made it unsafe. Do you think that's a fair assessment?

COHN: You mean a lack of money?

YUFFEE: Or, lack of desire to put enough money into the disposal of nuclear waste.

COHN: I think a large amount of money *has* been put into it. I mean, the design and the construction of those underground repositories in New Mexico, and the argument about using Yucca Flats[66] [in Nevada]. I think that money has been put into those, but as long as you have people who have the public-address system of the news media, saying, "It ain't safe, it's going to leak, it's going to get out, not in my back yard," no amount of money is going to override that. That's a belief in witches, and we've been through that once in our history. It's a superstition, that there's no safe way. After all, the Australians have a safe way, the Swedes have a safe way.[67] We've had people working out of Oak Ridge designing a safe way, or safe ways, for a long time. The political will isn't there.

YUFFEE: Maybe sites like Hanford, which have had problems with tanks leaking, and that sort of thing—

COHN: —Yeah, I know, but for every case like that there's a clear answer. Those [tanks] were built during World War II, when materials were at a premium, [making it necessary to build tanks from materials that were too thin or of lower grade]. Naturally those tanks are going to pot. Also, ask, "Where are they leaking to?" They're leaking into clays that sur-

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[66] the site selected in Nevada as the permanent repository for high-level spent commercial nuclear fuels

[67] Nuclear waste from Swedish commercial nuclear reactors is encased in special copper-clad glass capsules, which in turn are stored underground in stable granite formations.

round them, and the clays act like ion-exchange absorbers; so, it doesn't go anywhere once leaked.

So, yes, there were leaks. They won't be repeated. Also, the leaks haven't done any damage, and won't do any damage. The tanks built since then have been built differently, and don't leak. After all, you do learn from experience. The Chernobyl reactor was built along a design that was abandoned in this country decades ago, and what happened there can't happen here.

YUFFEE: And that concludes the interview. Thank you very much for your time. We appreciate your insight and your thoughtful comments and thank you for having us come here and talk to you. ❑